Reviewer Comments:

This letter to the Editor describes the experience in relation to COVID-19 vaccination in pregnancy in Vietnam. The authors collected two rather heterogeneous groups of pregnant women receiving one or two doses of Astra Zeneca or Pfizer COVID-19 vaccines and compared short-term adverse effects and pregnancy outcomes. The authors conclude with a statement supporting safety of both vaccinations with no major effects shown and no clear statement in relation to minor side effects found. In relation to obstetric outcomes, they described a higher proportion of women vaccinated with the Pfizer BioNTech vaccine with low birthweight infants compared to those vaccinated with the AstraZeneca product, and they raise a call for additional research on this topic.

I found the study topic and the research questions compelling and sound, however I find several major weaknesses and limitations mainly in methodology which are hindering both scientific relevance of the results and publication, in its present form.

Major

-A clear conclusion on minor side effects exposed in table 1 cannot be found in the manuscript.

Please add that

-There are statistically significant differences as far as the confounders are concerned (mean maternal age, double dose administration and medically assisted reproduction rates higher in Pfizer vs Astra Zeneca; nulliparity lower in Pfizer vs Astra Zeneca). There is no mention of essential covariates such as BMI, previous preeclampsia, chronic hypertension, and other covariates related to risk factors of major outcomes in analysis. Do you have these? Differences in some of the outcomes in analysis may well be explained by differences in the mentioned confounders and univariate statistics chosen are by far insufficient to depict any meaningful differences between such heterogeneous study groups.

-The study is grossly underpowered to estimated differences in rare events for some of the outcomes considered (malformations, stillbirth, maternal death). From this side no evidences are emerging.

-A critically important information is missing in this manuscript. How many women presented COVID-19 infection in pregnancy before or after the vaccine in the two groups? This is essential given the conclusion of the authors in relation to pregnancy outcomes and represents a major limitation to publish this paper.

The comment above is also essential in relation to abnormal pregnancy outcomes of women with covid 19 in pregnancy (references 1-3). I do not understand this?

-The authors appropriately subgrouped patients in cases with one or two doses at table 1, but this subgrouping disappears at table 2, limiting the usefulness of any difference given the major difference in rates of patients vaccinated with two doses bs one dose in the study groups.

-The difference of low birth weight between groups cannot simply be assessed being missing the information of median GA at birth and its statistical dispersion. In other words, if BW centiles are not different between study groups, the difference in the rate of low birth weight may well be explained by divergent GA at birth in the two groups.

There were prospective cohort studies on vaccinations in pregnancy, despite the authors statement in the introduction (references 4-7). In the majority of these studies the outcome was mainly related to immune response and not adverse effects or pregnancy outcome, however the authors must correct their statement and consider looking at previous experiences in detail. You should do that

In summary, in order to pursue publication in UOG or elsewhere I would recommend addressing all major issues mentioned herein in particular restructuring methodology either matching patients of study groups for major risk factors and major confounders or using multivariable statistical models capable of corrections for clinically relevant covariates.

References

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